Pitching Research

Robert W. Faff*
University of Queensland, Australia
r.faff@business.uq.edu.au

Abstract
This paper sets out a simple and systematic approach to pitching a new empirical research proposal. With a finance or accounting PhD student (or novice post-doc academic researcher) in mind, a template for the pitch is provided, with general advice on how best to apply it. A simple example is given in the area of capital structure to illustrate the specific application of the pitch template.

Keywords: new research ideas; pitching; accounting and finance; novice researcher advice

*contact author: UQ Business School, University of Queensland, St Lucia. Qld. 4072. Australia. Email: r.faff@business.uq.edu.au Telephone: +61 7 3346 8055 Fax: +61 7 3346 8166.

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1. Introduction

How can you know with any confidence that you have identified a good/worthwhile research topic? More to the point, how can you figure this out very early in the planning process so that you avoid unduly wasting precious time and resources on something that (sadly) will ultimately be seen as a "flimsy" addition to the relevant literature? The core objective of the current paper is to give tangible advice in this regard. My primary target audience is novice researchers in the finance and accounting disciplines – whether they be current doctoral students or novice researchers, with only limited publication experience in the very early phases of an academic career, post-PhD.

To this end, I propose some key guidelines to creating a sound research proposal. Specifically, I develop a simple "template" for the research proposal, in which you (the researcher) are challenged to concisely "populate" each section of the template with relevant material. To help motivate attention to this task, I ask readers to invoke a simple hypothetical context: imagine that you need to "pitch" your research proposal to a potential senior collaborator and that this person is extremely "time poor". Specifically, imagine that they can only devote 30 minutes of their time, to read and/or listen to you, to form a judgment on your basic idea. How would you go about meeting this daunting challenge? What areas/aspects should you cover? In what detail? How can you best package this information for efficient consumption and assessment?

The basic logic is to provide essential, brief information across a broad range of essential dimensions that any collaborator would need to make a reliable assessment of the quality of and potential for the proposal. Notably, it is assumed that the goal of this exercise is to produce a solid plan which, once executed, would eventually lead to a quality research paper – published as a fully refereed article in a highly reputable international academic journal.

There are numerous articles that give researchers general advice and valuable insights on how to get their research published and so such a perspective will not be repeated in any detail here. Two notable recent examples are Bradbury (2012) and Clarkson (2012).[1] One very interesting aspect of Bradbury (2012) is the provision of a highly useful pre-submission "checklist" covering some 40 different dimensions that will help authors improve their chances of publication. For example, his first checklist question asks: "Has the paper been presented as a conference or workshop …?", while a latter question in the checklist queries "Does the statistical analysis test the stated hypotheses?" (Bradbury, 2012, p. 356). Clarkson (2012) presents a companion piece that is more focused on the "editorial decision process". For example, Clarkson (2012, p. 363) enunciates a view of the "default mechanism" – "… the paper is rejected unless there is sufficient evidence to the contrary (i.e. sufficient evidence of merit and that the study can be brought to a publishable state with reasonable effort)."

A critical distinction exists between the objective/context of such "advice" papers versus the current paper. Most notably, articles like Bradbury (2012) and Clarkson (2012) assume that researchers already have a well-developed product (i.e. there exists a paper that is considerably beyond the first-draft stage), and the advice they then give is how to enhance and improve from this relatively advanced base. In contrast, in my paper, I am speaking to researchers who have embryonic notions which are yet to be formally explored, and for which the researcher is genuinely unsure of the underlying academic merit. Moreover, my focus is particularly on early career researchers (ECRs) since typically they are extremely challenged by a steep learning curve and often will feel like they do not even know where to start.

"Reverse engineering" the advice that authors like Bradbury and Clarkson provide, will work up to a point – however, many nuances and steps of "process" are hidden when a

[1] See the reference list for both of these papers that identify key earlier work broadly on this writing and publication advice. For example, Ashton (1998); Chow and Harrison (1998 & 2002); and Zimmerman (1998).

paper moves into its mature form. As such, several of the messages contained herein are lost, overwhelmed or never made explicit in the conventional "how to publish" articles. Moreover, the provision of a simple template for the "pitch" captures the essence of a tangible benefit that the reader gains from the current paper.

The remainder of the current paper evolves as follows. In Section 2 we outline the blank template and briefly guide the reader as to the underlying thinking behind each piece and how it might be completed. Section 3 illustrates the template by making a hypothetical pitch in the area of capital structure, while the final section concludes.

2. The Pitch Template

2.1 Preliminaries

What do you need to do to convincingly sell the merits of your idea to conduct a new empirical research project? I have designed a "pitch template" to help give a systematic answer to this question.[2] The pitch template is shown in Figure 1 in blank format, while Figure 2 repeats the template but now provides a series of prompting questions, as cues to induce the "pitcher" to cover a range of "likely suspects" under each heading.[3] I begin by discussing the components of the template and the basic philosophy/purpose behind each element. I also give some general guidance on how to populate each segment of the template. For ease of reference below, the key elements of the pitch template are labeled "Item (A) - (J)".

The first thing to understand about the design of the template is a need to be concise and to the point. It is very safe to assume that the "pitchee" [e.g. potential research collaborator, PhD supervisor, research mentor] is a very busy person. He/she is time poor and

[2] A softcopy WORD version of the template is available from the author upon request.
[3] Clearly, given the nature of the current exercise, the cues are not meant to be exhaustive or equally binding in every circumstance. Rather, the cues are indicative only and the pitcher should feel free to extend/curtail any aspects as deemed appropriate and most helpful to their overall cause.

in the first instance simply wants to know the essential ideas, without being bogged down by the details. With this in mind, my strong advice is to keep the completed pitch to a maximum of 2 pages. For a knowledgeable "pitchee", this limit will provide ample material to induce probing questions, leading to an informed judgment – and more detail can be called for once the pitch is deemed "successful"![4]

Indeed, the pitch can evolve. The very first version will very likely be rough and raw. This is expected. There is no shame in this. Rather, the shame will be if the "pitcher" is always too "scared" to share their pitch with their potential "pitchee" because they fear embarrassment. Air your ideas early, so that they might flourish or die – whichever is appropriate. Lost time is a lost opportunity. Should your early ideas flourish, the pitch template can form a useful framework for development across several iterations, until that moment of metamorphous is reached – when it is no longer a "pitch" – it becomes a fledgling project!

The template begins with stating the pitcher's identity – "ownership" is important.[5] The template then covers several broad essential ingredients of which the reader wants immediate knowledge: (A) working title; (B) the basic research question; (C) the key paper(s) and (D) motivation/puzzle.

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[4] There is no unique definition of "success" in this context. At one extreme, for a very early version of the pitch, success could simply mean that the senior collaborator wants to see a revised pitch that addresses some key areas in more detail. For an already heavily revised pitch, success would be indicated by the senior researcher agreeing to collaborate on the project, with an agreed division of duties on, for example, generating a detailed literature review and hypothesis development versus initial data collection and sampling – perhaps even staged via a "pilot" exercise.

[5] When it comes to "intellectual property" linked to research, a definitive statement of ownership is often problematic. Similar research ideas can be developed independently by different researchers – and it is quite possible that multiple "leaders" will be acknowledged in the literature. Perhaps one of the most famous cases in finance research is the development of the CAPM – many attribute this to the independent work of three academics: Sharpe (1964); Lintner (1965) and Mossin (1966). One way to stake an early claim to an idea is to make "public" your work in various forms as soon as possible e.g. by creating a working paper on SSRN; by delivering a research workshop at a university seminar program; or by presenting a paper at a recognised conference. Of course, if the idea is meritorious and potentially developed contemporaneously by several researchers, those who are too slow developing it to a mature state, risk being relegated as secondary players on the given issue.

2.1.1 Template Item (A): Working Title

The "first" challenge is to decide on a working title. While stated as the "first" challenge, in most cases the "working title" evolves over time. As such, the title can be refined several times during the process of completing the template and it becomes more clearly shaped as more information is gathered and cognitively processed. Indeed, you do not necessarily have to begin at the top of the template and work systematically down. The process is best thought of as a dynamic and iterative process, in which the "path" to a completed pitch is non-linear and unpredictable.

2.1.2 Template Item (B): Basic Research Question

The next challenge[6] is to capture in one sentence, the key features of the chosen research question. It is very likely that the research question will be very similar to the working title (Item (A)) – in some cases it might even be identical, but in most cases it will be subtly different, and slightly more expansive. While the question can take almost any form, it is typically "neutral" in its expression. Indeed it might not even be a question, in the literal sense. For example, it might be something like: *What are the economic determinants of "variable Y"*? or *To explore the empirical determinants of "variable Y"*. While such a research question does not identify any prediction(s) or hypothesis(es), it is readily connectable to the expression of such. Following on from the above example, the related hypothesis might be expressed as: *"Variable X" is a positive determinant of "variable Y"* (the opportunity to state a prediction/hypothesis comes later in the template under the Idea).

[6] Similarly, the "research question" evolves over time. The initial view is often rudimentary and overly simplistic, and it too becomes more clearly shaped as more of the plan comes together.

2.1.3 Template Item (C): The Key Papers

A sufficiently deep immersion within the relevant literature is essential to coming up with and confirming a good research topic. I use a frivolous metaphor to explain how to attack the literature challenge – the so-called “cocktail glass” approach. Imagine a fancy cocktail glass that is very broad at the top, narrows down to a small diameter – say, a third the way from the bottom and then fans out at the base – but much less so than the top. Such a glass is depicted in Figure 3. Drinking from the full cocktail glass is like beginning the literature search on a broad topic – there is typically a big literature to traverse, characterized by the big diameter at the top of the glass. As you spend time reading, filtering of the papers takes place, coincident with the refinement of the potential topic – quite likely an iterative process. Like the slow consumption of the cocktail (savoring the taste), the drink level heads toward the narrow part of glass – analogous to the narrowing in ones thinking about which papers within the relevant literature are the most important and critical foundation stones for your research topic. When you get to the narrow part of the glass, you have identified the small set of papers that really help you focus your attention on what is currently “known” and what is still unknown. These are the “key” papers. Later, should the project advance, an expanded set of the most relevant papers is identified as your reference list – like the cocktail glass, these represent the foundation upon which the paper (glass) rests.

I suggest that in answering the question posed in item (C) of the template – namely, what are the “key” foundational papers for your proposal, limit your answer to just three. Indeed, if possible nominate the most critical single paper to your work. You might ask: what “characteristics” should these critical paper(s) possess? First, they should be quite recent – say no older than 3 years. Ideally, they should be published in the Top Tier journals in the relevant field[7], or if they are not, then they should be very recent unpublished papers available on SSRN and authored by "gurus" in the relevant field. Collectively, all these conditions serve as heuristics for currency and quality.[8]

2.1.4 Template Item (D): The Motivation

The final "preliminary" consideration in the pitch template is the motivation (at item (D)). All high quality papers come with impressive motivation(s). This should emanate from the academic literature itself, but often it is also linked to observed agent behavior or actual industry patterns or real market imperatives or current regulation/policy debates. One really good strategy for motivating a paper is isolating a meaningful and relevant "puzzle" – which might be observed in recent market statistics that show curious trends or patterns or actual decision-making that defies conventional wisdom. For example, Driesprong, Jacobsen and Maat (2008) identify and explore the puzzling finding that changes in oil prices predict stock market returns worldwide. As a further example, Santa-Clara and Valkanov (2003) identify and explore the differential excess stock market returns for Democratic versus Republican presidencies.

The core of the template is built around the "gimmick" of a "3-2-1" design. "Three" represents the three essential ingredients of the Idea, the Data and the Tools. "Two" represents the two basic questions that a successful researcher always convincingly answers: "What's new?" and "So what?" "One" represents the "holy grail" – the Contribution! Ultimately the merits of any paper must stand on both its actual and perceived contribution to the literature.[9] Each element of the "3-2-1" design is discussed in the following sections.

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[7] Of course, in finance we have quartet of Tier 1 journals: the *Journal of Finance, Journal of Financial Economics, Review of Financial Studies* and *Journal of Financial and Quantitative Analysis*.

[8] Of course, any other objective means of telling that an unpublished paper will soon be an influential one in the Top Tier journals can be used – but the rules of thumb stated in the main text are reasonably safe suggestions.

[9] This section is strongly inspired by and very closely aligned to Section 2 of Faff (2013). Interestingly, in Faff (2013), the purpose at hand – namely, to assess a well-developed paper – is naturally compatible with the

2.2 Three Dimensions – Idea, Data and Tools

Any empirical paper has three critical dimensions: (1) the Idea; (2) the Data; and (3) the Tools. Faff (2013) proposes a "cheeky" acronym based on the first letters of Idea, Data and Tools – the so-called "IDioTs" guide to empirical research. While the three elements are, for expositional convenience, presented here as being independent of each other, in practice they are often interrelated considerations.

2.2.1 Template Item (E): The Idea

Absent a good idea, irrespective of how impressive everything else is, it is hard to imagine how a worthwhile paper can be created. As stated in Figure 2, against item (E) the main cue asks you to identify the core idea – the essential concept/notion/proposition that drives the intellectual content of your chosen research topic. Moreover, the template prompts for a brief articulation of the central hypothesis and also asks is there any theoretical tension involved? "Theoretical tension" reflects the situation in which there are meaningful contrasting predictions from two (or more) pockets of theory relevant to the research question. For example, Faff and Hillier (2005) exploit the theoretical tension emanating from informed trading vs. (in)complete markets vs. diminishing short sales, to test various trading characteristics of underlying equity upon the introduction of equity options.

While a critical aspect of a good research idea might very likely come from theory, the motivating idea might not necessarily be exclusively theoretical. As argued by Faff (2013, p. 952), "… the idea might involve an innovative blending of existing theory, or it might actually relate to a clever way of exploiting institutional differences or recognising unique exogenous events that allow reliable identification of causality. The idea might relate to the

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reverse the order of attack – "1-2-3". Ultimately, this reversal is innocuous – the essential elements and message remain robust.

identification of a "gap", for which we can't reliably deduce the answer from the existing literature."

2.2.2 Template Item (F): Data

An empirical paper without data is not an empirical paper. Item (F) in the template aims to expose key questions around the data and sampling, with a key focus on establishing feasibility of the project – both in terms of an adequate sample size ("quantity") and veracity of the data source/compilation ("quality"). By challenging the 2 "Qs", the current focus is centred on giving confidence that reliable inferences regarding the question at hand are ultimately deliverable. Item (F) of the template poses a (non-exhaustive) series of data-related questions. Question 1 largely prompts consideration of the chosen unit of analysis – both in a time series and a cross-sectional sense. Question 2 can in part be viewed as making us think about statistical validity, since sample size is a key factor. [10] Question 3, probes more on any likely (non-random) structure in the data – e.g. if the data have panel properties, the effective degree of independent observations is diminished from the "headline" pooled sample size. Question 4 is strongly asking us to confront feasibility – sources of data whether commercial or hand collected, pose potentially "deal breaking" issues in terms of prohibitive costs (either monetary or time). Questions 5 and 6 both connect to the veracity issue – missing data, or mixed up data or "unclean" data. All data are an unknown weighting of signal/information versus "noise", and concerns reflected in these questions can push the perceived noise/signal ratio beyond levels too high for comfort. As the old saying goes: "garbage in garbage out". Question (7) in this template item, asks us to contemplate any "other data obstacles?" While this could relate to anything, it helps prompt thoughts of other validity issues – e.g. external

[10] Clarkson (2012) argues that four dimensions of validity constitute the "cornerstone of scientific rigor": (a) internal validity – do we have a fully-specified model?; (b) construct validity – do we have compelling linkage between empirical proxies and economic variables?; (c) statistical validity – do we have appropriate data, sampling and tests?; and (d) external validity – will our results be generalizable?

validity: does the sample of data provide a representative and meaningful view of the underlying (and relevant) population?, or construct validity: are the feasible proxies compelling constructs for the underlying economic variables in question?

2.2.3 Template Item (G): Tools

Item (G) reminds us that without adequate tools/techniques, data and ideas are useless. A critical part of academic rigour is having systematic and formally designed statistical analysis that gives reliability/credibility to any/all inferences drawn. An empirical study that is purely descriptive or one that is based on univariate tests, will find little favour in the mainstream literature. In essence, the "toolkit" comprises the techniques, econometric models, software and so on, that collectively allows us to objectively "ask" the data for answers to the key research question and its related predictions/hypotheses. For examine, Item (G) asks the very basic question of whether a regression approach will be used – as is most common in the finance and accounting disciplines. Further, questioning what econometric software are fit for purpose, prompts the related questions of software availability and training. There is also a question of "connectivity" between tools and all other aspects of the proposed framework.

As emphasised by Faff (2013, p. 953) novel tools "… can provide added "leverage" to a research question, that helps create new insights not possible with standard techniques that are well-worn in a given literature. One example of such potential leverage is when a researcher transports an established technique from another discipline, and shows how it can give new insights, that for whatever reason are obscured by the existing "old" approaches."

2.3 Two Questions – What's New? and So What?

Yes, any IDioT can tell you that empirical papers are characterised by three critical dimensions: Idea; Data; and Tools. But, you can use these dimensions well or poorly – how can you plan to achieve the former and avoid the latter? I suggest the answer lies in two questions! First, ask yourself, what is new? Second, ask so what?

2.3.1 Template Item (H): What is New?

Faff (2013, p. 951-2) argues that a meaningful contribution should tell us something new, "… something that we did not already know based on an informed reading of the extant literature. If there is no novelty in the empirical work – for example, a straight replication of an existing paper, then it seems straightforward to conclude that there is no contribution." Moreover, Faff (2013) highlights that novice researchers often fall for the "trap" of taking a very literal interpretation of the word "new".

Perhaps the best example of this trap is the single country study, in which the relevant literature already documents clear and consistent evidence for country "X", country "Y" and country "Z", but nothing has been published in the author's chosen setting of country "A". Yes, in the narrow literal sense, generating a test for country A is new. However, the fallacy here is that an informed reader of these studies (with minimal effort) might be able to take a synthesised view of the collective extant literature and reasonably infer what will be applicable to country "A" (and, indeed, to a range of other similar countries). Thus, to establish meaningful novelty in such a single country study, the researcher needs to make a compelling case as to why it is dangerous to extrapolate the distilled evidence from X, Y and Z to country A (or to other similar jurisdictions).

Faff (2013, pp. 954-5) emphasises a simple device to help assess research novelty – the so-called "Mickey Mouse" diagram (i.e. Venn diagram). The idea is that based on a characterisation of the relevant literature, you define (at least) three circles of research attention that meaningfully overlap,[11] in ways that have not been completely explored in the extant literature. Figure 4 depicts a generic version of Mickey Mouse, in which two circles are at the top (i.e. "considerations" A and B) representing Mickey's ears and one circle at the bottom (i.e. "consideration" C) representing his head. Typically, for projects in which this characterisation makes sense, the zone of novelty is defined by the triple intersection i.e. "X marks the spot". An example illustrating an application of this device is shown in Section 3.

2.3.2 Template Item (I): So What?

One potentially fruitful way of successfully invoking a "novelty" dimension into a single country study is to identify some unique institutional feature or regulatory event(s) or unusual financial market behaviour relative to major developed markets. In finance, the common benchmark is the US setting – in all respects – its markets, regulatory setup and institutional designs. But simply being different to the US does not guarantee a fertile ground for new research. The critical reader (e.g. dissertation examiner or journal referee) will need to be convinced of the importance and relevance of these unique features to advancing knowledge in mainstream finance concepts/models? In other words, they will ask the "so what" question.

Accordingly, item (I) in the pitch template poses this very question, as a broad consideration – beyond the simple illustration posed above. Yes, lets assume that you have posed a novel research question. But, why is it important to know the answer? How will major decisions/behaviour/activity and so on, be influenced by the outcome of this research? If it is not sufficiently important, then no one will care. We should never embark on a research project that is effectively targeting a journal of "irrelevant results".

[11] There is no fixed requirement for what these circles might represent – they might be any combination of idea(s); data; tools; or relate to market features, regulation, … anything that makes sense. There is no right or wrong answer – just whatever works.

2.4 Template Item (J): One Contribution

The "holy grail" for any research is to make a contribution – this is NUMBER ONE. Thus, while few researchers would have trouble agreeing with this statement, no matter how experienced we become at doing research, the challenge of establishing contribution seemingly never becomes any easier. One reason for this is that as we become more experienced, we become more ambitious with our targeted journal – the higher the quality of the journal, the higher is the threshold standard for the required incremental contribution.

Thus, completing the penultimate section of the pitch template is bound to leave us all feeling unsatisfied or even a little disillusioned – but these are not good reasons to leave this item blank or for it to create a "road block". One comforting thought is that good responses to all of the previous parts of the pitch template, help to define the contribution. In other words, by the time you end up at Item (J), you have thought seriously about all the constituent parts needed for contribution. Now you are faced with the challenge of distilling this into a short statement about the primary force. Oftentimes, it will be inextricably linked to the Idea. But, the Data and the Tools will also play their part. Where is the essence of the novelty? Again, is the Idea new? Is there any novelty in the Data? Is it in the Tools? But, beyond novelty in any of these dimensions, what is the importance? Why should we care? This latter consideration can often invoke thinking around likely economic significance of possible findings. Finding statistical significance, absent economic significance is a hollow victory.

2.5 Template Item (K): Other Considerations?

Item (K) in the template is a residual or catchall – it presents a time for posing any other relevant final reflections. Various suggestions are offered in the template. Is collaboration needed/desirable? For doctoral students such a question will be a sensitive one – in many cases only limited collaboration will be permitted. What are your target journal(s)? Are these targets realistic? Sufficiently ambitious? Or, too ambitious? Also, what about a "risk" assessment? While totally subjective, can you make a judgment on whether the proposed project has "low" vs. "moderate" vs. "high" risk, in certain respects? For example, the risk of "insignificant results"? Or that of "competitor" risk (i.e. being beaten to publication by a strong competitor)? Or the risk of "obsolescence"? Is there any other major (research) risk exposure? Finally, are there any serious challenge(s) that you face in executing this plan? If so, what are they? Are they related to the Idea? The Data? The Tools?

3. A Pitch Example: Capital Structure

3.1 Preliminaries

Figure 4 presents a completed template for a hypothetical finance pitch, with a working title: "Explaining the Trade-off Theory Puzzle with a Unified Theory of Capital Structure" (Item (A)). The title gives a reasonable insight into what the key thrust is – an ambitious plan to, in some way, combine competing theories on capital structure into a "unified" design. In terms of item (B), the basic research question is clearly articulated: "Can we meaningfully articulate and test a "unified" theory of the capital structure decision?" In terms of pitch item (C), three key papers are identified: Warr et al. (2012); Faulkender et al. (2012) and Dang et al. (2012). Given that this hypothetical pitch was devised in early 2013, the "currency" issue mentioned earlier is satisfied. Further, two of three papers are published in the Top 4 finance journals – one in *Journal of Financial Economics* and the other in *Journal of Financial and Quantitative Analysis*. As such, the notion of quality "foundational" papers is satisfied. In terms of pitch item (D), the motivation/puzzle is expressed as a quote from Hovakimian and Li (2011, p. 44):

> "In the context of dynamic tradeoff models of capital structure with fixed adjustment costs and infrequent rebalancing, the magnitudes of the estimates suggest that it takes *more than ten years* for a firm to adjust to its target capital structure. These long adjustment times suggest that either *adjusting to target*

> *capital structure is not a high priority* goal for an average firm *or that the empirical models* currently used in the literature are *not well-suited* to identify the ways in which firms facing various tradeoffs manage their debt ratios. *Understanding the reasons* behind the relatively low economic importance of target debt ratios in partial-adjustment and debt-equity choice models *is a priority for future capital structure research*." [emphasis added]

The key elements of this quote are italicized, suggesting that a puzzle exits in the capital structure literature. Indeed, connecting to the working title of the pitch, the final entry in Item (D) of this pitch showcases the existence of the motivating "puzzle" with the question: "Why are there low SOAs (speeds of adjustment) when it seems that Target Leverage should and does matter?"

3.2 IDioT

Item (E) of the completed template identifies the "Core" idea. Suppose that a typical firm follows tradeoff theory in the long run, but pecking order (PO) and/or market timing (MT) in the short term. In empirical work, if we ignore this possibility, the estimated (overall) speed of adjustment parameter is biased downwards towards zero since it is an average of the positive speed of adjustment that applies to the scenarios applicable for tradeoff theory and the zero speed of adjustment that applies to the scenarios applicable to pecking order/timing. When TO/PO/MT theories are blended into a "unified" model ("UTOPOT"), the puzzle might be resolved. Item (E) of the pitch concludes with (i) a broad statement regarding the nature of the central hypothesis(es), namely, that they would comprise of a range of conditional hypotheses that capture the unified nature of the UTOPOT model; and (ii) highlighting the theoretical "tension", namely, to exploit the differential predictions of TO/PO/MT theories to identify conditions when each prevail/dominate.

Item (F) of the completed pitch template address many dimensions of the data. (1) identifies the US as the chosen country/setting; individual firms as the unit of cross-sectional analysis and annual sampling as the unit of time series analysis. (2) suggests an expected unbalanced pooled sample size exceeding 50,000 firm years encompassing the period 1951-2012 (current at the time of writing the original pitch). (3) the data sources are the usual suspects for this type of research (Compustat/CRSP/…), with no hand-collection of any data envisaged, no major time delays and relatively minor research assistance. (4) notes that these data are "standard" and recognised as high quality. (5) notes no major challenges/problems with the data/sample, but identifies the standard filtering practices e.g. excluding banks, winsorising, standard merge issues. (6) anticipates adequate power of the tests, in line with a mature prior literature.

Item (G) of the completed pitch template comments on the anticipated toolkit. It begins by noting that a relatively basic empirical framework of regressions built around the partial adjustment model forms the foundation, as standard in the literature. Dummy-variable and non-linear modelling, possibly including switching and or threshold type models. In terms of econometric software SAS and/or Stata are identified. The entry for Item (G) also clearly acknowledges a challenging empirical setup e.g. panel data modelling, endogeneity and clustered standard errors. Moreover, a "learning curve" and/or collaboration is flagged. Finally, this template item claims a compatibility of data with planned empirical framework – since it builds on a rich recent empirical literature applying similar models.

3.3 What's new? So what?

Item (H) in the completed pitch claims that the IDEA is novel by blending/unifying/integrating existing theories to explain Leverage Policy puzzle. The pitch further states that the data and Tools are standard. As such, the IDEA is claimed to be the "driver", while data/tools are STRONG passengers. Item (I) in the completed pitch responds to the "so what" question by arguing that getting a reliable answer to the chosen research question will help us better understand the behaviour of firms in making their capital structure decisions – under what circumstances do the incentives/drivers lead to a particular theory dominating the others and so, be consistent with maximizing shareholder wealth. The claim is that the research proposal gives a realistic chance of resolving a major puzzle – and perhaps play a part in restoring faith in corporate finance theories – collectively. Of course, the latter claim is quite extreme, and unlikely to ever be delivered upon. Aspirational goals like this still have value, especially if they are acknowledged as such.

3.4 Contribution

As already highlighted above, by the time we arrive at the “king” of all elements in the pitch template, Item (J), much of the hard work has been done with regard to spelling out the source/nature of the contribution. The contribution will have “DNA” links to the idea, to the Data, to the Tools. The contribution will be defined in terms of the novelty and the importance of the question posed. The contribution will not simply be the summation of all the parts – it will benefit from synergies maximized by a smart overall experimental design. In the completed pitch of Figure 5, the bottom line primary source of the contribution is claimed to be a simple idea that (helps) resolve(s) a big puzzle. If successful, this research will go a long way to “harmonising” the big 3 theories on the corporate financing decision.

3.5 Other Considerations?

The final item in the completed pitch template is Item (K), looking for any forgotten “snags” or obstacles. Regarding the question of whether collaboration is needed/desirable?, the answers are – idea: no; – data: no; – tools: maybe, in relation to switching/threshold modelling and sophisticated panel data and endogeneity issues. Regarding the target journal(s), an ambitious goal of Tier 1 finance. How realistic or unrealistic this target is, becomes a matter of judgment. The final entries in item (K) relate to the "risk assessment" exercise. It is claimed; (i) "no result" risk is low – theoretical tension between three theories justifies most outcomes, though some will be more interesting than others; (ii) "competitor" risk is medium/high – capital structure research is a very topical and crowded space; and (iii) risk of "obsolescence" is low – since the financing decision is a key pillar of the finance discipline with a pedigree exceeding 50 years.

4. Conclusion

This paper presents a simple template designed to allow a novice researcher to identify the core elements of a viable and worthwhile empirical research proposal. The template is built around the "gimmick" of a "3-2-1" design. **Three** represents the essential ingredients of Idea, Data and Tools. Two represents the two basic questions a researcher has to convincingly answer: "What's new?" and "So what?". **One** represents the "holy grail" Contribution! I hope that this template will be of great use as a training tool for developing strong research proposals by the leading researchers of the future.

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Figure 1: Pitching a New Research Topic – Blank "3-2-1" Pitching Template [2-page limit]

Pitcher's Name	
(A) Working Title	
(B) Basic Research Question	
(C) Key paper(s)	
(D) Motivation/Puzzle	
THREE	**Three** core aspects of any empirical research project i.e. the "**ID**io**T**s" guide
(E) Idea?	
(F) Data?	
(G) Tools?	
TWO	**Two** key questions
(H) What's New?	
(I) So What?	
ONE	**One** bottom line
(J) Contribution?	
(K) Other Considerations	

Figure 2: Pitching a New Research Topic – "3-2-1" Pitching Template with Cues [2-page limit]

Pitcher's Name	Your name here
(A) Working Title	
(B) Basic Research Question	IN one sentence, define the **key** features of the research question.[12]
(C) Key paper(s)	Identify the key paper(s) which most critically underpin the topic (just standard reference details).[13]
(D) Motivation/Puzzle	IN one short paragraph (say a max of 100 words) capture the core motivation – which may include identifying a "puzzle" that you hope to resolve.
THREE	**Three** core aspects of any empirical research project i.e. the "**ID**io**T**s" guide
(E) Idea?	Identify the "core" idea that drives the intellectual content of this research topic. If possible, articulate the central hypothesis(es). Is there any theoretical "tension" that can be exploited?
(F) Data?	(1) What data do you propose to use? e.g. country/setting; Why? Unit of analysis? Individual firms, portfolios; industries; countries …? sample period; sampling interval? Daily, weekly, monthly, quarterly, annual, … Type of data: firm specific vs. industry vs. macro vs. …? (2) What sample size do you expect? Cross-sectionally? In Time-series? (3) Is it a panel dataset? (4) Data Sources? Are the data commercially available? Any hand-collecting required? Timeframe? Research assistance needed? Funding/grants? Are there novel new data? (5) Will there be any problem with missing data/observations? Database merge issues? Data manipulation/"cleansing" issues? (6) Will your "test" variables exhibit adequate ("meaningful") variation to give good power? Quality/reliability of data? (7) Other data obstacles? E.g. external validity? construct validity?
(G) Tools?	Basic empirical framework and research design? Is it a regression model approach? Econometric software needed/appropriate for job? Accessible through normal channels? Knowledge of implementation of appropriate or best statistical/econometric tests? Compatibility of data with planned empirical framework? Is statistical validity an issue?
TWO	**Two** key questions
(H) What's New?	Is the novelty in the idea/data/tools? Which is the "driver", and are the "passengers" likely to pull their weight? Is this "Mickey Mouse" [i.e. can you draw a simple Venn diagram to depict the novelty in your proposal?]
(I) So What?	Why is it important to know the answer? How will major decisions/behaviour/activity etc be influenced by the outcome of this research?
ONE	**One** bottom line
(J) Contribution?	What is the primary source of the contribution to the relevant research literature?
(K) Other Considerations	Is **Collaboration** needed/desirable? – idea/data/tools? (either internal or external) **Target** Journal(s)? Realistic? Sufficiently ambitious? "**Risk**" assessment ["low" vs. "moderate" vs. "high": "no result" risk; "competitor" risk (ie being beaten by a competitor); risk of "obsolescence"; other risks? Are there any serious challenge(s) that you face in executing this plan? What are they? Are they related to the Idea? The Data? The Tools?

[12] The guidelines in red should be deleted and replaced by the best available "answers" in relation to the proposed research topic.
[13] Ideally **one** paper, but at most 3 papers. Ideally, by "gurus" in the field, either recently published in Tier 1 journal(s) or recent working paper on SSRN.

Figure 3: The Cocktail Glass Approach to Reading/Filtering the Literature

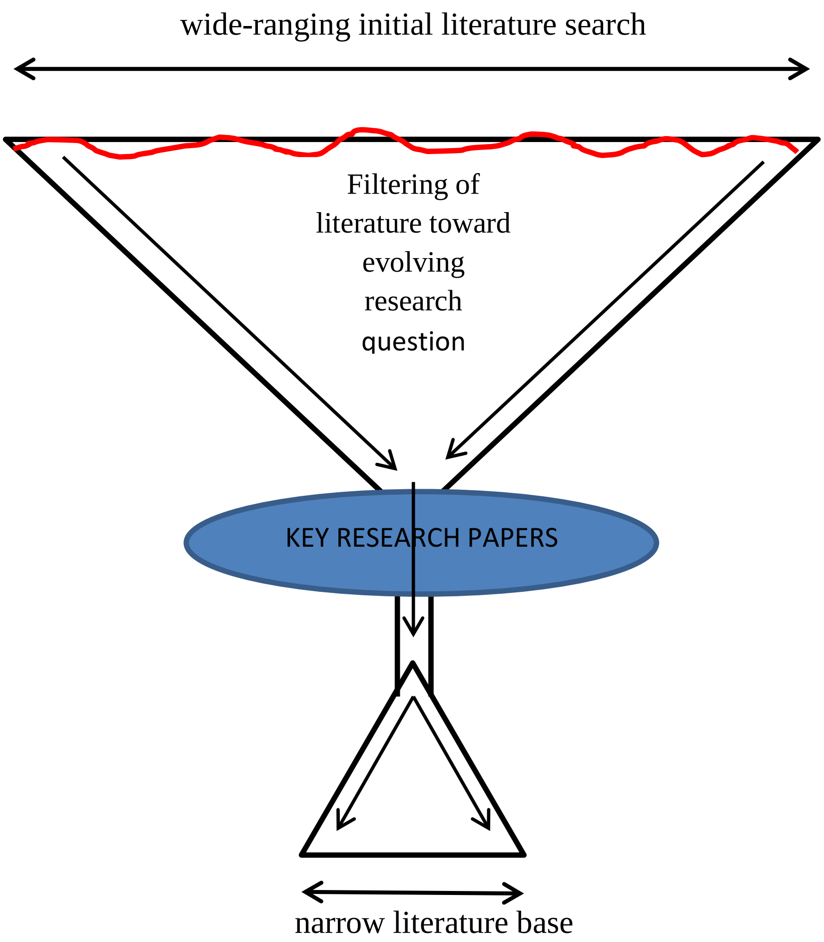

Figure 4: A Generic Characterisation of how Mickey Mouse helps us to identify Novelty in Research

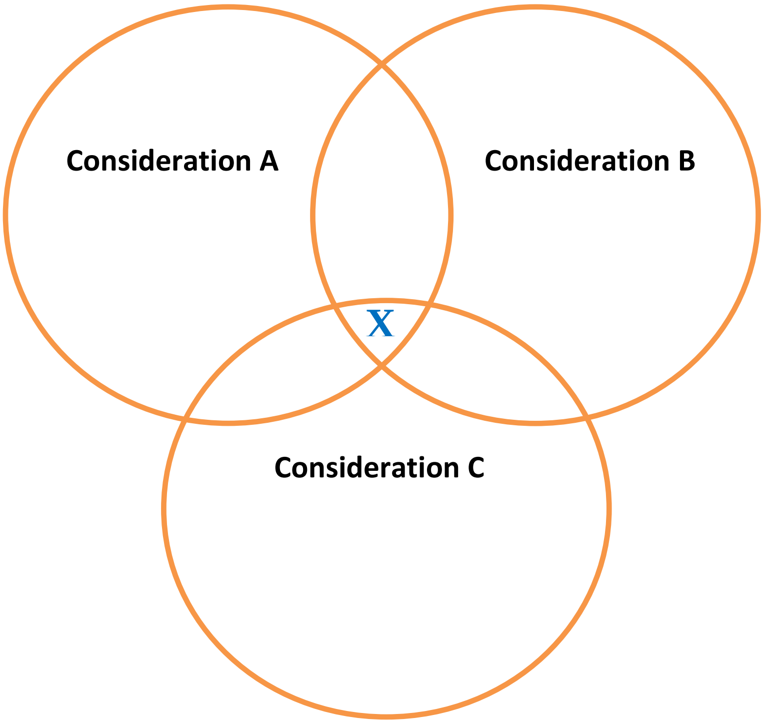

Figure 5: Example of "3-2-1" Pitching Template - Completed Pitch on a Capital Structure Topic

(A) Working Title	**"Explaining the Trade-off Theory Puzzle with a Unified Theory of Capital Structure"**
(B) Basic Research Question	Can we meaningfully articulate and test a "unified" theory of the capital structure decision?
(C) Key paper(s)	Warr, R., Elliot, W., Koeter-Kant, J. and Oztekin, O., (2012), Equity Mispricing and Leverage Adjustment Costs, Journal of Financial and Quantitative Analysis 47, 589-616. Faulkender, Flannery, Hankins & Smith (2012), Cash Flows and leverage Adjustments, Journal of Financial Economics, 103, 632-646. Dang, V., Kim, M. and Shin, Y., (2012), Asymmetric capital structure adjustments: New evidence from dynamic threshold models. Journal of Empirical Finance 19, 465-482.
(D) Motivation/Puzzle	Quoting Hovakimian and Li (2011, JCF, p. 44): "In the context of dynamic tradeoff models of capital structure with fixed adjustment costs and infrequent rebalancing, the magnitudes of the estimates suggest that it takes more than ten years for a firm to adjust to its target capital structure. These long adjustment times suggest that either adjusting to target capital structure is not a high priority goal for an average firm or that the empirical models currently used in the literature are not well-suited to identify the ways in which firms facing various tradeoffs manage their debt ratios. Understanding the reasons behind the relatively low economic importance of target debt ratios in partial-adjustment and debt-equity choice models is a priority for future capital structure research." **Puzzle:** Why are there low SOAs (speeds of adjustment) when it seems that Target Leverage should and does matter?
THREE	**Three** core aspects of any empirical research project i.e. the "**ID**io**T**s" guide
(E) Idea?	**"Core" idea:** Suppose that a typical firm follows tradeoff theory in the long run, but pecking order (PO) and/or market timing (MT) in the short term. In empirical work, if we ignore this possibility, the estimated (overall) speed of adjustment parameter is biased downwards towards zero since it is an average of the positive speed of adjustment that applies to the scenarios applicable for tradeoff theory and the zero speed of adjustment that applies to the scenarios applicable to pecking order/timing. When **TO**/**PO**/M**T** theories are blended into a "unified" model ("UTOPOT"), the puzzle might be resolved. **Central hypothesis(es):** a range of conditional hypotheses that capture the unified nature of the UTOPOT model **Theoretical "tension":** exploit the differential predictions of **TO**/**PO**/M**T** theories to identify conditions when each prevail/dominate
(F) Data?	(1) **Country/setting:** US,. Why? Because we can! Big bang for buck. **Unit of analysis:** individual firms. **Sampling:** annual. **Type:** mainly firm specific. (2) Expected **sample size:** > 50,000 firm years; **Cross-sectional:** several 1000's; **Sample period:** 1951-2012; unbalanced panel data (3) Data **source**(s): Compustat/CRSP/…? No hand-collecting required. **Timeframe:** given database subscriptions at UQ, no major time delays (1 week for core dataset); Research **assistance** needed?: "minor" assistance; Funding/**grants**?: not essential for viability, but potential opportunities; (4) Standard data – nothing novel, high **quality** data from Compustat/CRSP etc (5) Will there be any problem with **missing data**/observations?: nothing major, just standard issues – work through carefully eg banks exclusion, outliers & winsorising, standard merge issues etc (6) Will your **test variables** exhibit adequate ("meaningful") **variation** to give good power?: yes, since "blending" variables used in prior literature
(G) Tools?	Basic **empirical framework**: regression model approach focusing on partial adjustment, standard in the literature. Aim to enhance SOA model – via dummy-variable and non-linear modelling, possibly including switching and/or threshold models. Econometric **software** needed/appropriate for job?: SAS and/or Stata – licenses held at UQ. Panel data modelling, endogeneity and clustered standard errors etc make the setting complex BUT doable. Knowledge of **implementation** of appropriate or best statistical/econometric tests?: yes, but "learning curve" and/or collaboration **Compatibility** of data with planned empirical framework?: yes, building on rich recent empirical literature applying similar models

Figure 5 (continued)

TWO	**Two** key questions
(H) What's New?	**IDEA** is novel – blend/unify/integrate existing theories to explain Leverage Policy puzzle; data standard, tools standard IDEA is the "driver", and data/tools are the "passengers": US setting with half century of data – strong; leading edge application of panel data methods, probably encompassing switching/threshold methods – strong. Data/Tools are STRONG passengers.
(I) So What?	Getting a reliable answer to the question will help us better understand the behaviour of firms in making their capital structure decisions – in what circumstances the incentives/drivers lead to a particular theory dominating the others and so, consistent with maximizing shareholder wealth. It gives a realistic chance of resolving a major finance puzzle. Restores faith in corporate finance theories – collectively.
ONE	**One** bottom line
(J) Contribution?	**Primary source of the contribution:** simple idea that resolves a big puzzle. "Harmonises" big 3 financing decision theories.
(K) Other Considerations	Is **Collaboration** needed/desirable? – idea: no; – data: no; – tools: maybe, in relation to switching/threshold modelling and sophisticated panel data and endogeneity issues? Target Journal(s)? Tier 1 finance. Realistic? Yes, given Warr et al (2012, JFQA). "**Risk**" assessment: – "no result" risk: LOW – theoretical tension between three theories justifies most (all?) outcomes, though some will be more interesting than others; – "competitor" risk (ie being beaten by a competitor): MEDIUM/HIGH – is very topical and crowded research space – need to keep an eye out for key academics in this area eg authors of key papers above; – risk of "obsolescence": LOW – financing decision a key pillar of the finance discipline > 50 years since M&M gave birth to modern finance theory; – other risks?: too big? [solution – collaboration, PhD topics later?]